

## ***Interactive comment on “An evaluation of analytical streambank flux methods and connections to end-member mixing models: a comparison of a new method and traditional methods” by M. Exner-Kittridge et al.***

### **Anonymous Referee #1**

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Review of “An evaluation of analytical streambank flux methods and connections to end-member mixing models: a comparison of a new method and traditional methods” by Exner-Kittridge et al.

This paper derives a new mathematical method for estimating gross exchanges between the stream channel and lateral water based on in-stream tracer concentrations. The new method is based on the assumption of a uniform spatial distribution of both gross gains and gross losses along the reach.

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## General comments:

There are a couple fundamental issues the authors should resolve before this paper is accepted in any form for publication. First, I would like to see a more careful theoretical explanation of the meaning of equations 21 through 24 in the physical context. There is an apparent discontinuous artifact of the derivation that is not acknowledged and draws some doubt on the validity of the assumptions of simplifications made during the derivations. Second, the authors need to make their physical connotations and subsequent conclusions clearer in section 4.2. As written, this derivation seems to largely reflect a self-evident truth and otherwise be identical to the derivations earlier in the paper.

Regarding eq 21-24: The derivation arrives at equations where  $\ln(Q_{\text{final}}/Q_{\text{init}})$  is in the denominator. These equations are clearly discontinuous at the point where  $Q_{\text{final}} = Q_{\text{init}}$ , which is the case when  $Q_{\text{in}}=Q_{\text{out}}$ . I can think of no physical interpretation of why it would be impossible to estimate  $Q_{\text{in}}$  and  $Q_{\text{out}}$  when there is no net change in flow over the reach. Is this an artifact of the assumptions made? Is it the result of the arbitrary removal of the  $dC/dQ$  term between eq 14 and 15? The authors need to acknowledge this characteristic of their final equations and assess its implication to the theory and how to work around it in practical applications.

Regarding section 4.2: I cannot follow how the exercise with the mixing model is appropriate for a stream reach, and the results of the exercise appear to be combination of a self-evident fact and a repeat of the derivation of equation 5. Their treatment of the variables does not resemble the “streambank flux scenarios”. Foremost, they assume there is some sort of loss of the gross gain (or  $s_2$  in equation 36) that apparently happens before mixing with channel water (as evident in the load term for  $s_2$  in equation 37). Then, in eq 40 they derive the solution of the equations for the gross gain that occurs only after this loss. This can only mean that this particular loss of water cannot possibly have any effect on the tracer concentration in the channel and the fact that it falls out of equation 40 is self-evident, thus the mathematic argument becomes a tau-

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tology. If you remove this loss of gross gain from the exercise, the whole thing becomes identical to the derivation of equation 5, just with different subscripts on the variables. In the end, trying to understand section 4.2 was an interesting academic exercise, but I do not see a substantial contribution that makes it easier to understand the implications of gross exchanges on channel solutes. In fact, it appears to be largely a more convoluted version of mathematics that are already presented quite clearly early in the paper. I do not see the authors' apparent wish to criticize the McGlynn and Covino (2007) paper and the Briggs et al. (2012) paper as justifying this exercise. Plus, I think representing equation 40 as somehow conceptually different from equation 5 is due largely to at best unclear (and at worst faulty) logic.

Should these be resolvable issues, I suggest the authors reconsider their variable naming scheme.  $Q_{init}$  and  $Q_{final}$  imply variability in time. Yet their whole derivation is based on steady-state assumptions and  $Q_{init}$  and  $Q_{final}$  represent the upstream and downstream flows respectively. Why not call these  $Q_{up}$  and  $Q_{down}$ ?  $Q_{in}$  and  $Q_{out}$  are too non-specific. I suggest that the authors make it clearer which input and output these refer to. Perhaps  $Q_{gain}$  and  $Q_{loss}$ ? These variables are defined clearly enough in Figure 1, but the whole paper would be far more intuitive if the variables were named more consistently with the words used in the text. Finally, the interesting parts of the analysis are based on the fact that we are looking at the minimum, maximum, and somewhere-in-between estimates of gross exchanges. Wouldn't use of  $Q_{in, min}$  and  $Q_{in, max}$  rather than  $Q_{in, L-G}$  and  $Q_{in, G-L}$  make this comparison among variables easier?

I suggest the authors de-emphasize the idea of the compared methods being more or less "accurate" relative to reality. The minimum and maximum cases represented by the loss-gain and gain-loss estimates are specifically intended to bracket the true values (i.e. not necessarily to be "accurate"). The fact that another method that is always going to be somewhere between these two is generally more accurate is almost a foregone conclusion. What is interesting about the analysis is largely represented

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nically in Figure 6, which makes it clear that the gain-loss method is generally likely to overestimate more than the loss-gain method is likely to underestimate.

Specific comments:

Title – Is “streambank flux” a general enough term for the applicability of the mass balances in this paper? I see no reason why the applicability of this method should be limited to analysis of exchanges with the “bank”. Also, “comparison of a new method and traditional methods” provides little information about the nature of the analysis. How about “comparing estimates of gross exchanges based on different assumptions about spatial flow distribution”.

Pg 10430 lines 7-13: All the derivations presented are dependent on stationarity in tracer concentration over time. During a slug tracer test, the tracer concentration is far from stationary. If the authors are going to suggest that the derived equations are appropriate for use with slug tests, they need to make it clear how the lack of stationarity does not affect calculations using their approach.

Pg 10432 lines 1-3: Why 1000 and 2000 meters? This seems much longer than the reaches used in typical (and practical) artificial tracer tests. Was it based on the spatial statistics (averaging 100 to 200 m switching lengths) from the papers based on distributed temperature sensing? There are many places where the authors can be more specific about physical connotations of this theoretical and mathematical exercise.

Pg 10440 line 2: This statement is backwards relative to Figure 6. Loss-gain is the minimum and gain-loss is the maximum.

Figure 5: The Monte Carlo simulations of different distribution of gross exchanges appears to result in an exclusion of overall gains and losses of smaller values (very few values less than 1 L/sec in  $Q_{out}$  and  $Q_{in}$  graphs). Would including more scenarios with smaller overall exchanges change any conclusions?

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 10419, 2013.

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